The amphibia are so nearly allied to the ganoid fishes, that we can hardly doubt their descent from some member of that group. With our present limited knowledge of the extinct forms, however, it would be unprofitable to attempt to trace in detail their probable genealogy.

The authors to whom especial credit is due for our knowledge of American fossil fishes and amphibians, are Newberry, Leidy, Cope, Dawson, Agassiz, St. John, Gibbes, Wyman, Redfield, and Emmons, and the principal literature of the subject will be found in their publications.

Reptiles and birds form the next great division of vertebrates, the sauropsida, and of these the reptiles are the older type, and may be first considered. While it may be stated with certainty that there is at present no evidence of the existence of this group in American rocks older than the carboniferous, there is some doubt in regard to their appearance even in this period. Various foot-prints which strongly resemble those made by lizards, a few well preserved remains similar to the corresponding bones in that group, and a few characteristic specimens, nearly identical with those from another order of this class, are known from American coal measures. These facts, and some others which point in the same direction, render it probable that we may soon have conclusive evidence of the presence of true reptiles in this formation, and in our overlying permian, which is essentially a part of the same series. In the permian rocks of Europe, true reptiles have been found.

The mesozoic period has been called the age of reptiles, and during its continuance some of the strangest forms of reptilian life made their appearance, and became extinct. Near its commencement, while the triassic shales and sandstones were being deposited, true reptiles were abundant. Among the most characteristic remains discovered are those of the genus *Belodon*, which is well known also in the trias of Europe. It belongs to the thecodont division of reptiles, which have teeth in distinct sockets, and its nearest affinities are with the crocodilia, of which order it may be considered the oldest known representative. In the same strata in which the belodonts occur, remains of dinosaurs are found, and it is a most interesting fact that these highest of reptiles should make their appearance, even in a generalised form, at this stage of the earth's history. The dinosaurs, although true reptiles in all their more important characters, show certain well marked points of resemblance to existing birds of the order *Ratitæ*, a group which includes the ostriches; and it is not improbable that they were the parent stock from which birds originated.

During triassic time, the dinosaurs attained in America an enormous development both in variety of forms and in size. Although comparatively few of their bones have as yet been discovered in the rocks of this country, they have left unmistakable evidence of their presence in the foot-prints and other impressions upon the shores of the waters which they frequented. The triassic sandstone of the Connecticut Valley has long been famous for its fossil foot-prints, especially the so-called "bird-tracks," which are generally supposed to have been made by birds, the tracks of which many of them closely resemble. A careful investigation, however, of nearly all the specimens yet discovered, has convinced me that there is not a particle of evidence that any of these fossil impressions were made by birds. Most of these three-toed tracks were certainly not made by birds: but by quadrupeds, which usually walked upon their hind feet alone, and only occasionally put to the ground their smaller anterior extremities. I have myself detected the impressions of these anterior limbs in connection with the posterior foot-prints of nearly all the supposed "bird-tracks" described, and have little doubt that they will eventually be found with all. These double impressions are precisely the kind which dinosaurian reptiles would make, and as the only characteristic bones yet found in the same rocks belong to animals of this group, it is but fair to attribute all these foot-prints to dinosaurs, even where no impressions of fore-feet have been detected, until some evidence appears that they were made by birds. I have no doubt that birds existed at this time, although at present the proof is wanting.

The principal genera of triassic reptiles known from osseous remains in this country are, *Amphisaurus* (*Megadactylus*), from the Connecticut Valley, *Bathygnathus*, from Prince Edward's Island, *Belodon* and *Clepsysaurus*. Other generic names which have been applied to foot-prints and to fragmentary remains, need not be here enumerated. A few remains of reptiles have been found in undoubted Jurassic rocks of America, but they are not sufficiently well determined to be of service in this connection. Others have been reported from supposed Jurassic strata, which are now known to be cretaceous. It will thus be seen that, although reptilian life was especially abundant during the triassic and Jurassic periods, but few bones have been found. This is owing in part to the character of most of the rocks then formed, which were not well fitted for preserving such remains, although admirably adapted to retain foot-prints.

(*To be continued.*)

ON NOCTURNAL INCREASE OF TEMPERATURE WITH ELEVATION[1]

TILL the year 1862, when my first experiments were made by the use of the balloon, our knowledge of the temperature of the air was almost entirely confined to within four or five feet of the earth's surface, and the theory that the temperature was always lower at high elevations, and that the decrease of temperature with increase of elevation was at the rate of 1° Fahrenheit for every 300 feet of elevation, was generally received and acted upon. These theories were found not to be at all times true, and the assumption of the decrease of 1° of temperature in every increase of 300 feet of elevation was proved to be erroneous in every balloon ascent I have made; in some a decrease of 1° and more than 1° was experienced within 100 feet, and there is no doubt that, considering the quickness of motion on leaving the earth, the decrease at such times was really 2° or 3°, or more, within the space of 100 feet.

In some of the ascents a difference of 10° was met with within 1,000 feet of the earth, whilst in others but little or no difference was experienced even to heights exceeding 1,000 feet.

Towards the end of my balloon experiments it was evident that a very large number more were necessary, and in my last report I said:—

From all the experiments made it would seem that the decrease of temperature with increase of elevation is variable throughout the day, and variable in different seasons of the year; that at about sunset the temperature varies but very little for a height of 2,000 feet; that at night with a clear sky the temperature increases with elevation; that at night with a cloudy sky there was a small increase of temperature as the height increased; that in the double ascent of May 29, 1866, the one just before and the other after sunset, it would seem that after radiation from the earth began, the heat passes upwards till arrested where the air is saturated with vapour, when a heat greater by 5° was experienced after sunset than at the same elevation before sunset.

This was the state of our knowledge when M. Giffard most kindly placed the great "Captive" balloon, located at Ashburnham Park, Chelsea, near London, at my disposal for a series of experiments.

This balloon could ascend to the height of 2,000 feet on a calm day; its rate of ascension could be regulated at will; it could be kept stationary at any elevation, and experiments could be repeated several times in the day.

On two different days I ascended nine times on each day; there was a decrease of temperature with increase of elevation at every ascent, but, different in amount at every hour, being less and less as the day advanced towards sunset. The results of the experiments are shown in the following table, showing the amount of decrease of temperature per 100 feet of elevation, at different hours of the day with a clear sky, and a cloudy sky, as found by experiments with M. Giffard's captive balloon.

Height above the ground.		Clear Sky.						Cloudy Sky.				
From	To	10 A.M. to 11 A.M.	3 P.M. to 4 P.M.	4 P.M. to 5 P.M.	5 P.M. to 6 P.M.	6 P.M. to 7 P.M.	About 7 P.M.	3 P.M. to 4 P.M.	4 P.M. to 5 P.M.	5 P.M. to 6 P.M.	6 P.M. to 7 P.M.	7 P.M. to 8 P.M.
feet.	feet.	°	°	°	°	°	°	°	°	°	°	°
0	100	1·0	1·4	1·2	0·9	0·5	0·0	1·2	1·2	0·8	0·6	0·5
100	200	1·0	0·8	0·7	0·7	0·5	0·1	0·9	0·7	0·6	0·6	0·5
200	300	0·9	0·7	0·6	0·7	0·5	0·2	0·9	0·6	0·6	0·6	0·5
300	400	0·9	0·6	0·5	0·7	0·5	0·3	0·7	0·5	0·6	0·5	0·5
400	500	0·8	0·6	0·4	0·6	0·5	0·3	0·5	0·4	0·5	0·5	0·5
500	600	0·7	0·5	0·4	0·5	0·5	0·3	0·5	0·4	0·5	0·5	0·5
600	700	0·6	0·5	0·4	0·4	0·5	0·4	0·4	0·4	0·5	0·5	0·5
700	800	0·6	0·5	0·4	0·4	0·5	0·4	0·4	0·5	0·5	0·5	0·5
800	900	0·5	0·5	0·4	0·4	0·4	0·4	0·4	0·4	0·4	0·5	0·5
900	1000	0·5	0·5	0·4	0·4	0·4	0·4	0·4	0·4	0·4	0·5	0·4

[1] Abstract of a paper read at the Havre meeting of the French Association by Mr. James Glaisher, F.R.S.

This series of experiments proved that which was only indicated in the ascents with a free balloon, viz., that the change of temperature with increase of elevation has a diurnal range, the change being the greatest at about midday and the early afternoon hours, decreasing till about sunset, at which time, when the sky was free from clouds, there was little or no change of temperature up to the height of several hundred feet. I was not able, by means of M. Giffard's balloon, to take any observations at about noon and early afternoon hours, nor any observations after sunset, as the balloon never ascended at these times; but such observations were greatly needed, as there seemed to be at this time a very high probability that the temperature of the air at night must increase with elevation.

A thermometer was placed at the height of 22 feet, sufficiently protected from the effects of radiation, and a second one at the height of 4 feet, and eleven years' observations of these instruments have been taken daily at 9 A.M.; noon; 3 P.M.; and 9 P.M.

These observations were reduced by taking the difference between the readings of the two thermometers and affixing the sign + to that difference when the temperature was higher at the higher elevation; and the sign – when lower. By taking the mean differences for each month between the temperatures at 22 feet and 4 feet, it was found that at all hours of the day during the months of January, February, November, and December, in the afternoon hours of March, September, and October, and night hours throughout the year, the sign was + and that it was – at all other times, clearly indicating the fact of an increase of temperature with increase of elevation during the night hours throughout the year.

THE HEAT PHENOMENA ACCOMPANYING MUSCULAR ACTION

THE fact that in the living muscle heat always appears when the muscle does work (Heidenhain having shown that of two muscles equally weighted and undergoing equal contractions, one doing external work, while the other does none, the former gives out more heat than the latter), is an exception to the general rule in mechanics, that heat disappears when work is done. It is not, however, in contradiction to the general principle of the conservation of energy, but shows that in the living muscle, when stimulated to action, molecular processes occur, which, along with the doing of work, cause a development of heat. The relation of the heat developed to the work done had not been determined with any satisfactory accuracy, probably owing to the want of sufficiently delicate apparatus, though it might naturally be expected to help to an understanding of the phenomena. The subject has been taken up by M. Nawalichin, who, favoured by the experimental means at hand in M. Heidenhain's Physiological Institute, made a careful examination of the development of heat in the active muscle. The experiments were very difficult and tedious, and by reason of the smallness of the values to be measured, required very great foresight and care in the experimental arrangements. The full account of this investigation is given in *Pflüger's Archiv*.

The first series of experiments bore on the question of the production of heat when a particular muscle of the frog is excited, through the nerve, by stimuli of increasing strength to increasing contractions. As, during the experiments, the excitability of the preparation varies, the relation to the strength of stimulus was left out of account, and only the ratio between development of heat and height of contraction examined. The height of contraction was indicated graphically by the muscle itself on a smoke-blackened plate. The development of heat was measured by the deflection of a fine thermo-multiplier, and the stimulation of the nerve was effected by accurately measurable electric actions. The observations were only made when the needle was entirely at rest, which was very difficult to secure, so sensitive was the apparatus.

The tabulated numbers from experiment show: (1) that the sum of the *vis viva*, liberated in the muscle by increasing stimuli, increases only so long as the lifting-heights (*Hubhöhen*) increase. With a certain amount of stimulus when produced by the sending of a constant current, the height of contraction reaches a maximum, and therewith, too, the production of heat. With a particular method of stimulation there is, under certain conditions, a fresh increase of the amount of contraction above the maximum amount, the so-called "supermaximal" contraction; where this occurred, the heat-production also rose. It may therefore be said that in general the development of heat increases with increased lifting-height, and decreases with decreased lifting-height.

The increase in heat-production, however, does not take place proportionally to the increase in lifting-height, but in much quicker ratio. Of this unexpected result M. Nawalichin assured himself by repeated discussion of the numerical values obtained; but he did not succeed in determining more precisely the law of increase.

This result led to the expectation that the same mechanical work of a muscle would be accompanied by unequal heating when the muscle raised a weight to the same height by several small contractions, and when it raised it by one great contraction. In a great contraction more heat would become free than in several small ones, the sum of which was equal to the great. Experiments (though some were difficult) fully confirmed this, especially after it was ascertained that the cooling during the longer period of the several smaller contractions as against the shorter duration of the great contraction, did not play a part.

It is shown, then, that as the stimulation increases, the temperature of the muscle, and accordingly the exchange of material, increase in much quicker ratio than the mechanical work, and that the stronger the stimulation the less favourable is the relation of the exchange of material to the doing of work.

These facts are in accordance, as M. Nawalichin points out, with the common experience that the climbing of a hill is much less heating and exhausting when we go zigzag than when we go straight up. In the former case a greater number of small liftings of the body result in the same doing of work as occurs in the second case through a smaller number of great liftings. The exchange of material, as the second series of experiments show, must essentially be greater in the second case than in the first; and on the amount of it depends, on the one hand, the development of heat, on the other the exhaustion.

In order to get at the inner connection of the phenomena observed, M. Nawalichin sought first to decide the question whether the accelerated increase in production of heat was due to the increase of the stimulus in itself or to the increase of the contraction produced by the increased stimulation. According to Helmholtz's observations, when a muscle is subjected to two maximum stimuli, one following close on the other, the second stimulus produces an increase of contraction only when, at commencement of the second contraction, the first has already reached a considerable height. If this be not the case, as happens if the interval of the two stimuli be less than $\frac{1}{600}$th of a second, the two stimulations produce no greater contraction than each alone. Now in what way does the production of heat occur in this latter case? Experiment showed that also with double stimulation of the nerve, an increase of the heat-development only occurred when it had as result an increase in the height of contraction; the increase of the stimulus in itself is thus without influence on the amount of heat-production. Hence the cause of the quicker increase of the heat-production. That of the amount of contraction must be sought in conditions operative during the course of the contraction.

To determine these conditions the author made experimental inquiry into the relation of heat-development to the states of tension of the muscle during the progress of contraction. He found that the muscle developed less heat the less its tension before action; with which may be mentioned that this tension of the muscle, weighted and stretched by the weight, is smaller the more it has, through contraction, approximated to the natural length. Experiments, also, as to the relation of the heat-production to the change of state of tension during the act of contraction showed an influence of this, such that in each moment of action the quantity of heat depends on the tension. This suggested the idea that the greater heat-production with increasing stimulation is perhaps a consequence of the longer duration of the stronger contraction. The experiments proved, however, that this idea is not justified, for the muscle made small and great contractions in the same time.

As to the nature of the internal processes in the muscle, which may be the basis of the phenomena observed, M. Nawalichin offers the following remarks:—

"We know that the contracting muscle is a body of variable elasticity; with increased contraction its elastic force becomes less, its extensibility greater. When the muscle raises a given weight about four millimetres, the external work for each millimetre of the lifting-height is indeed the same but nevertheless the